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The Respective Roles of Human and Nonhuman Subjects in Behavioral Research

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Let me make it clear that I have a great deal of respect for the body of work published by Baron, Perone, and Galizio and that I agree with much of what they have to say in the present instance. Indeed, I am puzzled to find that they regard me as having taken an "extreme" stand in opposition to the use of human subjects in behavioral research. I think that this is a misreading of what I have said. The passage that they quote was taken from an exchange, similar in format to the present one, between myself as the author of the target article (Dinsmoor, 1983) and Perone and Baron (1983) as the authors of one of the commentaries. In the target article, I had cited a number of studies, mainly from the pigeon literature, that supported the fundamental principle that "information," in the form of a negative discriminative stimulus (S−), does not reinforce observing responses. In their commentary, Perone and Baron cited data from a previous publication (Perone & Baron, 1980) in which they "found that S− reinforced the behavior of adult humans, indicating that information can function as a reinforcer" (Perone & Baron, 1983, p. 714). It was to that challenge that I was responding.

In that context, my response was necessarily brief, not a detailed exposition of my point of view. In selecting the material they quoted, Baron et al. concentrated on a single, although central, theme to the exclusion of other aspects of what I had to say. I argued against "attributing the [Perone and Baron] results to a biological discontinuity between the inborn behavioral propensities of primates and pigeons" and suggested that because of the influence of past experience "the human species [is not] a suitable one for the investigation of fundamental behavioral processes .... On the other hand," I quickly added:

> Human beings are obviously the species we would most like to know about. If Perone & Baron intend to pursue the question of how their subjects originally acquired the pattern of behavior demonstrated in the 1980 experiments, I wish them every success. The topic is an intriguing one. (Dinsmoor, 1983, p. 719)

I also directed my readers to an earlier publication (Dinsmoor, 1970, pp. 1–3) in which I had discussed the choice of species at greater length (see also Dinsmoor, 1952, 1960). Unfortunately, the 1970 publication is not widely available, and Baron et al. may have suffered some difficulty in obtaining it.

For the record, I do *not* "object to human research in general" (Baron et al.). In the original article from which their quotation is taken I cited with approval several human studies, including one in which I myself served as one of the authors (Mulvaney, Hughes, Jwaideh, & Dinsmoor, 1981).

WHAT CONSTITUTES A FUNDAMENTAL BEHAVIORAL PROCESS?

Part of the difficulty may be that I had not defined, other than by the problem at hand, what I meant by "fundamental behavioral processes." But note that Perone and Baron also drew a distinction between historical influences and "a basic principle of behavior" (1983, p. 714). In the passage Baron et al. have cited, I had in mind the initial generation and subsequent refinement of broadly applicable, presumably innate principles of behavior, like those involved in stimulus generalization, the formation of a discrimination, the differentiation of a response, schedules of reinforcement, chaining, conditioned reinforcement, and so on. Skinner's *The Behavior of Organisms* (1938) is an appropriate exemplar. I do not think it would have made sense

for Skinner to have conducted that research with human subjects.

On the other hand, once the basic principles have been worked out with rats and pigeons, I do think it is appropriate to conduct additional tests with human subjects. In one sense, this is "applied" research, because the purpose is to see whether these principles also apply to the species with which we wish ultimately to use them. But if the behavior that serves as the dependent variable is selected on technical grounds, as an abstract indicator for the behavioral processes under investigation rather than for its significance to society, the research could in that sense be classified as "basic" research. And I also think there are some behavioral processes, such as responding to verbal instructions and forming equivalence classes, that do not reach full expression with nonhuman subjects and must therefore be studied with humans.

DIFFERENCES IN EMPHASIS

I suspect that the differences between Baron et al. and myself are in part, although not entirely, a matter of emphasis. Both of us are valiantly defending the kind of research in which we have chosen to invest our own energies, and it is natural that despite large areas of agreement we tend to highlight different aspects of the problem. Evidently Baron et al. think that the use of human subjects is under attack and they are eager to defend it. Those of us who work with nonhuman subjects can sympathize with Baron et al., for we too feel that we are the victims of discrimination. We too think we receive much less support and recognition than we deserve.

Rightly or wrongly, the lay public has much less respect for psychological or behavioral expertise than it has for scientific authority in the harder sciences. In psychology, it thinks it already knows part of the answer. And it supports what it understands. The public tends to look with favor on principles, like those in cognitive psychology, that require only minor refinements or extensions of conceptual foundations laid down during the highly suggestible years of infancy and childhood but to resist principles, like those coming from the conditioning laboratory, that require the formulation of a new set of concepts to deal with the subject matter.

Similarly, the general public tends to resist research, even on the most basic of behavioral principles, conducted with nonhuman subjects. Students in undergraduate classes frequently complain that they are not interested in the behavior of "animals," which they regard as very different from human beings. Unless they have been exposed to a laboratory course in which they have had an opportunity to see for themselves how behavior can be molded by manipulating its consequences, they find it difficult to take seriously the proposition that, for technical reasons, the path to an understanding of human nature lies through the use of nonhuman subjects. Congress, state legislatures, granting agencies, university administrators, and even many of our own departmental colleagues are essentially extensions of the general public and reflect the same point of view. Resulting difficulties in obtaining jobs, research funding, increases in salary, tenure, and promotion exercise a chilling effect. We are also the targets of assaults by animal rightists, many of whom object to any use of nonhuman subjects in any form of research.

It is my impression that most of the students trained in research with nonhuman subjects eventually switch to some more remunerative form of employment, such as computers or applied psychology, and this, too, constitutes a significant drain on the laboratories that train them. Some, to be sure, continue to work with nonhuman species, but in developmental, pharmacological, or physiologically rather than behaviorally oriented research. People like Skinner, Pavlov, Watson, Thorndike, and Hull, whose reputations rest primarily on learning work with nonhuman subjects, have been ranked at the very top of their profession in terms of their influence on psychological theorizing (Wright, 1970). Yet as Alexandra Logue (1986) has pointed out in

her review of O'Donnell's *The Origins of Behaviorism* (1985), the same problem has been faced throughout the history of psychology. In the early years, there were only two major figures working with nonhuman subjects, John B. Watson and Robert M. Yerkes. Until he switched to advertising, Watson complained bitterly and repeatedly about the penury from which he suffered (Cohen, 1979), and Yerkes was unable to place his students in academic positions (Logue, 1986; O'Donnell, 1985). All that rescued nonhuman research was the perceived relevance of the principles of learning to the field of education, which was obviously an important area of application. Similarly, I believe that today the future of the type of research Skinner conducted depends to a large extent on recognition of the contributions it has made and can continue making to the technology known as behavior modification. Many people do not know where the original ideas were generated. Public understanding of the linkage is essential.

THE ARGUMENT FOR USING NONHUMAN SUBJECTS

The argument for the use of nonhuman subjects for the initial determination of behavioral relationships rests on several grounds:

1. Serious ethical questions are raised if, merely to see what happens when a particular procedure is employed, we subject a human being to experimental conditions that are to any significant degree harmful to his or her best interests as an individual. Supporters of the animal rights movement would have us extend the same principle to other species, making a large array of medical and behavioral experiments completely impossible, but most members of our society are willing to balance the costs to nonhuman subjects against the benefits to the human species.

2. Many conditioning experiments, if properly done, take a long time to complete. It is relatively easy to bring subjects housed in an animal colony back for as many sessions as we wish but very difficult to do the same with free-ranging human subjects.

3. With the possible exception of money—which can quickly make the project prohibitively expensive—we have nothing to offer human subjects that is comparable in effectivenss to the food or water we use as reinforcers with other species. It is difficult to establish reliable experimental control.

4. The rats used as experimental subjects were bred and raised for that very purpose and should be extremely homogeneous, both in their genetic backgrounds and in their life histories. The pigeons may have been raised for other purposes, but they are also the products of controlled breeding and a uniform environment (Levi, 1957). Human subjects, on the other hand, are drawn from a much more diverse population, and because of their complex learning histories in an uncontrolled natural environment they are likely to differ much more in their patterns of behavior.

5. Perhaps most important, nonhuman subjects are normally reared and housed between experimental sessions in environments that have been stripped to their bare essentials. The laboratory rat or the pigeon has little chance to learn anything beyond the most primitive laws of physics and biology—how to get from here to there, what is edible, and so on. Although the ability to find and consume the reinforcer may provide a foundation for subsequent conditioning, in other respects it seems unlikely that the subject has learned much that can influence its behavior in the conditioning session.

But human subjects are not raised under comparable conditions. In modern cultures they are schooled, informally and formally, in a variety of skills that may be of interest to the cognitive or to the educational psychologist but not to one who is trying to investigate the fundamental behavioral characteristics of the living organism. At home and in the school system they are trained to describe situations in words, including counting, estimating time (frequently, subjects are asked to surrender their watches, which may focus attention on this dimension),

identifying colors, naming notes on the musical scale, categorizing objects, events, and spatial or temporal relations, and so on. This training leads to a great deal of mediated generalization and discrimination (Miller & Dollard, 1941, pp. 71, ff). One problem of particular significance is that the word "experiment" (or even the use of a laboratory setting) establishes a special type of social relationship in which most of us attempt to be a "good subject," that is, to divine the purpose of the experiment and to respond in such a way as to support the experimenter's hypothesis (Orne, 1962). On the other hand, in cases where deception is suspected some subjects may attempt to outwit the experimenter.

Human subjects are also trained in a variety of problem-solving techniques like comparing and contrasting similar items, testing hypotheses (see Levine, 1975), remembering past outcomes (see Buchwald, 1969), making deductions according to logical rules, and using systematic procedures to eliminate alternative solutions. Sometimes the subjects' learning histories lead to behavior quite different from that envisioned by the experimenter (e.g., Keller, 1977, p. 21). Some subjects report that they took the experiment to be a test of such matters as their powers of extrasensory perception or their ability to withstand discomfort or frustration. All of these previously learned responses are contaminants that threaten to conceal or to distort the original, natural behavioral propensities of the human organism. Given these difficulties, what seems remarkable is not that discrepant results are sometimes obtained but how often the human data turn out to be consistent with those from other species.

BARON ET AL.'S REBUTTAL

In answer to the criticism that human behavior is likely to be distorted by long and complex learning histories outside of the laboratory, Baron et al. set forth two major points. First, they suggest that the influence of past history (and of the vagaries of verbal instruction, interacting with that history) can be reduced by the use of a steady-state design, in which the subjects are exposed to the relevant contingencies for a substantial number of sessions before the critical data are collected. This makes sense to me. I agree that it is a desirable strategy. But it is very difficult to persuade human subjects to return as often as needed to the conditioning laboratory, and most research with human subjects is based on a very small number of sessions.

Furthermore, in this context the steady-state design is only a palliative strategy, which may or may not take care of the problem. As Baron et al. have granted, "there can be no guarantee ... that steady-state procedures will effectively counteract the influences of extra-experimental variables." And in our original exchange, for example, Perone and Baron (1983) favored an interpretation of their (1980) steady-state data as a product of the past history of their subjects rather than as a reflection of a basic principle of behavior. I feel much more secure when the subject has begun the experiment with a minimal learning history, even when a steady-state design has been employed.

Second, Baron et al. point out that even with nonhuman subjects the past history may have an influence. In this connection, they point to an experiment by Reynolds (1961) in which two pigeons responded differently for unknown reasons, an experiment reported by Thomas (1969), in which pigeons responded differently following prolonged exposure to a training tone in the housing area, and an experiment by Hebb (1949) in which rats reared in enriched environments responded differently from rats raised in laboratory cages. The point is interesting and important. But I think there is a difference here between humans and other species. With rats and pigeons we have a choice: If we wish to use an enriched environment as an experimental variable, we can do so, but if we prefer to minimize the contribution made by prior learning we can maintain our subjects in an environment that has not been enriched. Human subjects, on the other hand, are regularly and inevitably ex-

posed to learning environments that would most certainly be classified as enriched by the standards used for laboratory animals. Very few members of the human species are reared under conditions remotely comparable to those that are considered normal for the rat or the pigeon.

A SPECIAL ROLE FOR HUMAN RESEARCH

As indicated earlier, I do think there is a role for human research at a level that is certainly not "applied" in the full sense of the word and might be considered "basic" in at least one sense. Baron et al. discuss the problem of persuading people outside of the behavior analytic fold. The average person is not readily convinced that "animal" data apply to human behavior, and critics do capitalize on the discordant results that sometimes arise when human subjects are employed. I heartily concur with Baron et al. in their statement that "also needed is compelling evidence that the basic principles, heretofore discovered with animals, really do operate in human behavior."

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